severity—proximal isolated stenoses versus multiple stenoses with distal vessel involvement—are features of coronary artery disease that strongly influence surgical strategy, complexity of the operation, and prognosis. Moreover, the quality of coronary vessels and graft availability and quality determine the completeness of the revascularization and its durability. With the matching method adopted by Brown and colleagues,[1] these data are lost to the analysis, making it impossible to evaluate any effects on the higher perioperative and long-term mortalities observed in the bioprosthesis group. Higher mortality could be related to an unfavorable preoperative status. In that case, the surgeons who performed the operations may have shown good clinical judgment, implanting bioprostheses in patients with poor prognosis. Moreover, there are no data on prosthesis sizes, effective orifice areas, and the incidence of patient–prosthesis mismatch, all of which could influence long-term results. In conclusion, Brown and colleagues deserve commendation for the insights from their interesting study. Nevertheless, extreme caution should be exercised in supporting or questioning existing clinical guidelines in response to potentially biased retrospective studies.

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Reply to the Editor:

Drs Cannata, Russo, and Taglieri commented on our results and the possible impact of selection bias, which is present in all observational surgical series. We matched patients and used multivariate analyses, but it is impossible to determine whether this has accounted for all possible confounders. We agree that only randomization can distribute unmeasured covariates evenly between groups.

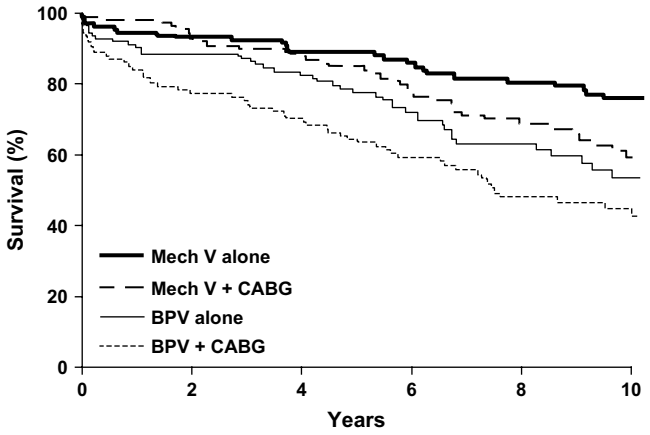

Figure 1. Survivals in mechanical aortic valve *(Mech V)* and bioprosthetic aortic valve *(BPV)* groups according to presence or absence of coronary artery disease, as indicated by coronary artery bypass grafting *(CABG)*.

A question was raised regarding the extent of coronary artery disease. We used coronary artery bypass grafting as a surrogate for coronary artery disease, and the mean numbers of grafts per patient were 1.0 ± 1.3 in the mechanical aortic valve group and 1.0 ± 1.2 in the bioprosthetic aortic valve group ($P = .86$). We performed an analysis of survival of patients undergoing aortic valve replacement with and without coronary artery bypass grafting (Figure 1). The survival benefit we observed with a mechanical aortic valve appears, however, to be independent of the presence of coronary artery disease in our multivariate model, as shown in Table 5 of our original article.[1]

Our finding of a survival benefit in the mechanical aortic valve group was surprising, and we have hypothesized that one possible explanation would be patient–prosthesis mismatch. We do not have the effective orifice area data available for this study group. We have, however, included information regarding body surface area and body mass index, size of prosthesis, and the incidence of aortic root enlargement in each group (see Table 1 in our original article). In general, surgeons at our clinic perform aortic root enlargement to accommodate a larger prostheses in any patient who is at risk for patient–prosthesis mismatch.[2]

We believe that current guidelines are generally appropriate for mechanical or bioprosthetic valve selection. Our primary concern is with the growing trend toward placing bioprosthetic aortic valves in younger patients. All observational (retrospective or prospective) studies are unable to eliminate the potential impact of patient selection bias; however, this investigation adds equipoise.

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doi:10.1016/j.jtcvs.2008.06.012

Importance of stabilization of the mitral annulus in mitral valve repair

To the Editor:

We read with great interest the article of Flameng and coauthors[1] about their experiences with mitral valve repair in Barlow disease and fibroelastic deficiency. They found that not using annuloplasty ring in mitral valve repair is a risk factor for recurrent mitral valve regurgitation. We

agree completely that the mitral annulus should be supported somehow, but our experience suggests that suture annuloplasty gives good results without any ring.

After our initial experience with suture annuloplasty, published 8 years ago,[2] we performed several hundred mitral repairs without annuloplasty rings and with good early and midterm results. There are two important factors in achieving a good result with this method. The first is the suture material. Both polypropylene (Prolene; Ethicon, Inc, Somerville, NJ) and polyester (Ethibond; Ethicon) sutures were not reliable. Prolene was too elastic, and in some cases the 3-0 Prolene thread elongated, causing redilatation of the mitral annulus. In a few cases, the Prolene suture ruptured, causing recurrent mitral regurgitation. Ethibond was difficult to use in achieving proper and equal tensions on the suture line and adequate shortening of the annulus. We had superior experiences with expanded polytetrafluoroethylene (Gore-Tex; W. L. Gore & Associates, Inc, Flagstaff, Ariz) sutures, as also reported by others.[3] The other important factor is that the sutures be placed properly into the annulus. Neither sutures in the left atrium in the vicinity of the annulus nor sutures in the proximal parts of the valve cusps give good results. This should hold true even with ring annuloplasty.

Suture annuloplasty actually has advantages relative to ring annuloplasty. Transvalvular gradient is significantly lower and mitral valve area is significantly larger, without differences in mitral regurgitation.[4] The mitral annulus has a natural flexibility, which results a 26% dilatation in diastole.[5] The natural flexibility of the mitral annulus is conserved with suture annuloplasty. Suture annuloplasty is also a simpler and much cheaper method of achieving stability of the mitral annulus.

In our experience, if mitral repair is done, mitral annuloplasty should be performed, even if the annulus is not significantly dilated. It is not absolutely necessary to use an annuloplasty ring, as Flameng and coworkers recommend[1]; suture annuloplasty can give a good result as well. Their statement that "[N]onuse of annuloplasty ring is a risk factor for recurrent mitral valve regurgitation" would be true only if the comparison included suture annuloplasty as well. We have not compared suture annuloplasty with annuloplasty rings, but in accordance with our experience, we would recommend the following conclusion instead: "Lack of stabilization of the mitral annulus increases the risk of recurrent mitral valve regurgitation."

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doi:10.1016/j.jtcvs.2008.05.026

Reply to the Editor:

I thank Szentkirályi, Péterffy, and Galajda for their interest in our study on the durability of mitral valve repair in Barlow disease. The point they try to make in their letter is that mitral annuloplasty in myxoid mitral valve degeneration does not necessarily have to be a ring annuloplasty. This might be correct; who knows?

The problem is that, nowadays, when you make a statement, hard data are required to convince the medical and scientific community to agree with you. In our study on the recurrence of mitral valve regurgitation after successful repair of Barlow disease and fibroelastic degeneration, we presented and analyzed extensive preoperative, intraoperative, and postoperative data, including long-term echocardiographic follow up. In our study, patients received either a ring annuloplasty or no annuloplasty at all. We did not study any other form of annuloplasty, and therefore it would not be correct to draw such a general conclusion as, "Lack of stabilization of the mitral annulus increases the risk of recurrent mitral valve regurgitation." To advocate this statement, one should test every possible form of annuloplasty and compare them all with no annuloplasty. In their own experience, Szentkirályi, Péterffy, and Galajda mention that some materials used for suture annuloplasty, such as polypropylene and polyester sutures, cause recurrent regurgitation. Is this not why the annuloplasty ring was developed?

Before we conclude that "Lack of stabilization of the mitral annulus increases the risk of recurrent mitral valve regurgitation," I invite Szentkirályi, Péterffy, and Galajda to analyze their data on suture annuloplasty in a scientific way and compare them with results of repair procedures without annuloplasty, rather than relying on their own experience, even with several hundred patients. I am well aware of the importance of personal experiences of renowned surgeons such as our Hungarian colleagues, and I appreciate the opportunity to share these experiences. Before introducing a concept into current praxis, however, I prefer data-based, scientific proof.

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Questions on the angular (Gothic) arch configuration and its applicability after type I aortic dissection repair

To the Editor:

I enjoyed the recent article "Angular (Gothic) Aortic Arch Leads to Enhanced Systolic Wave Reflection, Central Aortic Stiffness, and Increased Left Ventricular Mass Late After Aortic Coarctation Repair: Evaluation With Magnetic Resonance Flow Mapping," by Ou and associates[1] in a recent issue of the *Journal*.[1] Why were patients